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VOL. LVII MARCH 9, 1923 No. 1471

The Physical Basis of Life: PROFESSOR EDMUND B. WILSON 277

Darwin and Pasteur—an Essay in Comparative Biography: THE LATE WILLIAM THOMPSON SEDGWICK 286

Edward Emerson Barnard: DR. OLIVER J. LEE 289

Scientific Events:
The Haber Process; International Scientific Cooperation; Research Service to the Industries of Michigan; Public Lectures of the Chicago Section of the American Chemical Society; Organization of Scientific Men in the Northwest; The Meeting of the American Chemical Society in New Haven 291

Scientific Notes and News 295

University and Educational Notes 299

Discussion and Correspondence:
The Feeding Power of Plants: DRS. A. R. DAVIS, D. R. HOAGLAND and C. B. LIPMAN.
The Personal Equation of the Astronomers: PROFESSOR KNIGHT DUNLAP 299

Quotations:
Copernicus 302

Scientific Books:
Howarth on the British Association for the Advancement of Science: PROFESSOR BURTON E. LIVINGSTON 303

Safranin and Methyl Green: DR. H. J. CONN 304

Special Articles:
Furfural as a Biological Reagent: DR. C. E. THARALDSEN 305

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THE PHYSICAL BASIS OF LIFE[1]

I HAVE been much honored by the invitation to speak on this occasion, but for me it has meant more; for the man in whose memory the Sedgwick Memorial Lectureship has been established was my life-long and cherished friend. My theme to-day is drawn from an infinitesimal but all-including world, bounded by the horizon of the compound microscope, a world that may seem far distant from Sedgwick's own broad domain of sanitary science and the public health. I am sure, however, that such would not have been his own view; for Sedgwick was one of the pioneer teachers of general biology in this country, and it was his life-long habit to think of the phenomena of life in terms of the activities of protoplasm.

I have a lively recollection of how he and I, in the days of our youth, when fellow students at Yale, fell under the spell of Huxley's Edinburgh address on the "Physical basis of life," at that time still a subject of widespread popular discussion. In this celebrated discourse, delivered in 1868, the eminent English biologist set forth certain general conclusions concerning protoplasm which had gradually taken shape through the work of such investigators as De Bary, Max Schultze, Kühne, Brücke and Lionel Beale. Huxley's presentation of the subject was a masterpiece, both of English style and of philosophical breadth of outlook. In part for this reason, still more because of its supposed materialistic implications, it aroused immediate and widespread public attention. Huxley himself warned that to accept his conclusions would be "to place one's foot on the first rung of a ladder which in most people's estimation is the reverse of Jacob's and leads to the antipodes of heaven"; nevertheless, he insisted that

[1] The first Sedgwick Memorial Lecture, delivered in Boston, December 29, 1922. It will hereafter be published in fuller form, with illustrations, under the auspices of the Department of Biology and Public Health, Massachusetts Institute of Technology.

he was individually no materialist but on the contrary believed materialism to involve grave philosophic error. Despite this disclaimer, his conclusions aroused a storm of criticism and protest which came to a climax a few years later when Tyndall, in the famous Belfast address, proclaimed his faith in non-living matter as offering the "promise and potency of every form of terrestrial life." Huxley's heresy of sixty years ago has become an orthodox platitude to-day; but the problems of protoplasm still hold us fast with a gripping interest that has lost nothing of its force with the flight of time. In what light do Huxley's conclusions appear after the biological progress of half a century?

It is necessary to bear in mind that those conclusions were formulated before modern cytology had been born, and long before the cell had been clearly thought of as a colloidal system. From our present point of view we employ the word protoplasm as a collective term to designate the substances that constitute the active or living materials of which cells are composed. I use the plural form, substances, advisedly; for it is here in the first place that Huxley's statements now require recasting in more modern terms. To many readers his discussion conveyed the impression that protoplasm is a single chemical substance or "living protein." In his opening words he speaks of the physical basis of life as "some one kind of matter common to all living beings." He pictures a union of lifeless substances, such as water, ammonia and carbon-dioxide, to form "the still more complex body, protoplasm"; and the properties of this substance, he affirmed, must result from the nature and disposition of its molecules. "The thoughts to which I am now giving utterance," said Huxley, "and your thoughts regarding them, are the expression of molecular changes in that matter of life which is the source of our other vital phenomena."

In one sense, no doubt, these words are true; but evidently they do not express the whole truth. Long ago it became perfectly plain that what we call protoplasm is not chemically a single, homogeneous substance. It is a mixture of many substances, a mixture in high degree complex, the seat of varied and incessant chemical transformations, yet one which somehow holds fast for countless generations to its own specific type. The evidence from every source demonstrates that the cell is a complex organism, a microcosm, a living *system*. With the microscope we distinguish in this system a clear ground-substance or *hyaloplasm* in which are suspended a great variety of formed bodies, widely diverse in form and function, each of which plays its own particular part in the activities of the system. Examples of these are the nucleus, the cytoplasmic chondriosomes and plastids; the Golgi-bodies and central bodies, and many kinds of granules and fibrillæ. Some of them seem to be permanent, others to be transitory formations that come and go in the kaleidoscopic operations of cell-life. Which of them are alive? Which, if any, constitute the physical basis of life? What, in other words, is protoplasm?

These are embarrassing questions. One of the most pleasing functions of the teacher of elementary biology is to demonstrate to the laboratory student the substance of a living cell and assure him cheerfully that he is beholding protoplasm; and by good luck it rarely occurs to the disciple to cross-examine his master on the subject. Were it otherwise, how many a bad quarter of an hour might we have to endure! For the truth is that the more critically we study the question the more evident does it become that we can not single out any one particular component of the cell as the living stuff, *par excellence*. Of this fact most experienced cytologists, including such eminent leaders as Flemming, Strasburger, Bütschli, Kölliker and Heidenhain, long since became convinced. "No man," said Flemming, "can definitely say what protoplasm is . . . In my view that which lives is the entire body of the cell." It is this view of the physical basis of life that has impressed us more and more as our knowledge of the cell has advanced; and this is as true of the physiologist and the chemist as of the cytologist. I quote a distinguished biochemist. "We can not," says Professor Hopkins, "without gross misuse of terms, speak of the cell life as being associated with any particular type of molecule. Its life is the expression of a particular dynamic equilibrium which obtains in a polyphasic system. Certain of the phases may be separated, but

life is a property of the cell as a whole, because it depends upon the equilibrium displayed by the totality of coexisting phases." This conclusion is in substance precisely the same as that of the cytologist.

I repeat, therefore, that when we speak of protoplasm as the physical basis of life, we mean simply the sum total of all the substances that play any active part in the cell life; and we can not, I think, exclude from the list such substances as water and inorganic salts which we commonly think of as "lifeless." At first sight this may seem a rather barren conclusion; but the fact is quite otherwise. No conception of modern biology offers greater promise of future progress than that the cell regarded as a whole is a colloidal system, and that what we call life is, in the words of Czapek, a complex of innumerable chemical reactions in the substance of this system. Modern investigation has indeed already profited so much by the point of view thus offered as to suggest that the study of protoplasm and the cell may be destined to pass more and more into the hands of the physiologist, the physicist and the chemist. In any case the rising tide of cell-research in these directions is of good augury for the future experimental analysis of vital phenomena. There are, however, other aspects of the problem which still escape the precise quantitative methods of the physicist and chemist, or are only beginning to come within their range, but none the less are essential to our view of the general problem. I refer to those phenomena with which the cytologist, the embryologist and the geneticist must try to deal; and it is especially to this side of the question that I here ask attention.

The cytologist is first of all struck by the extraordinary pains that nature seems to take to ensure the perpetuation and accurate distribution of the components of the system in cell-division, and hence in heredity. Nothing is more impressive than the demonstration of this offered by the nucleus of the cell; but its obvious meaning has often been disregarded or treated with a blind scepticism which pretends that no meaning exists. To our limited intelligence, it would seem a simple task to divide a nucleus into equal parts. The cell, manifestly, entertains a very different opinion. Nothing could be more unlike our expectation than the astonishing sight that is step by step unfolded to our view by the actual performance. The nucleus is cut in two in such a manner that every portion of its net-like inner structure is divided with exact equality between the two daughter-nuclei; and the cell performs this spectacular feat with an air of complete and intelligent assurance. The nuclear substance is spun out into long threads or chromosomes; these are divided lengthwise into exactly similar halves; they shorten, thicken, separate and pass to opposite poles; and from the two groups formed are built up two daughter-nuclei, while the cell-body divides between them. In outward appearance such a process seems to contradict all physical principles, but its meaning has now become perfectly plain. In a general way it means, as Roux pointed out forty years ago, that the nucleus is not composed of a single homogeneous substance, but is made up of different "qualities" or components; and it means that these components are strung out in linear alignment in the threads so that they may be divided and distributed in a particular manner through the longitudinal doubling of the threads.

This conclusion led the way in a series of investigations that have brought forth some of the most notable discoveries of our time. The direct cytological evidence of a serial alignment of smaller bodies along the nuclear threads has thus far indicated the fact in only a somewhat rough and ready fashion, showing hardly more than the fact that the nuclear threads may often be seen to contain smaller bodies or "chromomeres" aligned in a single series, and sometimes showing definite size-differences. It seems certain, however, that this visible structure is no more than the rough expression of a finer one that lies beyond the reach of the microscope; and fortunately genetic experiment has here come to the rescue with indirect evidence on a grand scale, derived from experiments on the mechanism of Mendelian heredity. This evidence was brought forward mainly by Morgan, Sturtevant, Bridges and their coworkers in their widely known studies on heredity in the fruit-fly *Drosophila*, an object which offers unparalleled opportunities for extended and accurate experiment owing to the readiness with which it can be bred under standardized conditions, the remark-

able speed of its development, and its frequent production of heritable mutations. I regret the necessity that limits my reference to this remarkable work to a bare indication of its most general results. It has brought a final demonstration of the fact, established in a more general way by earlier observers, that the nuclear threads or chromosomes seen during cell-division play an essential part in the process of hereditary transmission. It has removed every doubt that the Mendelian phenomena may be fully explained by the behavior of the chromosomes or their components (as was first indicated in a more general way by Sutton, Boveri and DeVries).

It has brought overwhelming confirmation of the correctness of Roux's conception of the nuclear threads as linear aggregates of specifically different smaller entities of some sort, we know not what. For all this we had in some degree been prepared by earlier researches; but what now follows seems at first sight, I confess, completely incredible. The evidence drives us on to the conclusion that even the smallest details of heredity depend upon the behavior of infinitesimal units or "genes," far more minute than the chromomeres, strung out in linear series in the nuclear threads, each of its own specific kind and self-perpetuating by growth and division. It has made clear the fact that in the conjugation and disjunction of these bodies lies the ultimate explanation of Mendel's fundamental law. And, finally, in the midst of our struggles to assimilate all this, we are dealt a final blow with the remorseless demonstration that these units must be of definite number, separated by fairly definite and constant intervals, and *arranged in a definite and invariable serial order!* When we try to reckon with this series of demands, we find ourselves fairly gasping for breath. Such results are indeed staggering—to a certain type of mind even harder to believe than those which physicists are now asking us to accept concerning the structure of atoms. Nevertheless, *they are probably true!*

It is necessary to emphasize the fact that these conclusions did not arise in the fertile imagination of a Bonnet, a Buffon, or a Weismann. They are the product of concrete and extended experiments under carefully controlled conditions; they have made possible precise and quantitative prediction; and the data can be confirmed by laboratory experiment almost as readily as those with which the physicist or the chemist has to deal. In these respects they are comparable in validity with many of the concepts of physical science. It is possible, I suppose, to consider such conclusions as nothing more than a convenient fiction or algebraic symbolism, a kind of ideal model by means of which the genetic facts may conveniently be grouped. Those, however, who prefer to take their point of departure in the observed cytological facts will be more likely to make use of the actual model which every dividing cell displays to us in visible reality —a model that is not less impressive because at present the cytologist sees it only in broad outline with no more than dim indications of the finer complications inferred from the results of genetic research. And in point of fact it was this actual model from which came the first suggestions for the foregoing conceptions, and thus made possible some of the most important experimental researches on heredity in our time. Even if considered only as working instruments, therefore, these conceptions have a practical value almost comparable to that of the atomic theory as employed in chemistry and physics.

Cytology and genetics have thus combined to make real to us the existence of an organization of the nuclear region of the cell-system that is as complex and wonderful as any pictured by the fantasy of the speculative nature-philosophers. But we can not rest content with this demonstration. Inevitably we are led on—perhaps I should say led back—to the question whether an organization of similar type, or in any degree approaching to such a type, may also exist in the cytosome or extra-nuclear region of the cell-system. Conservative cytological opinion has been extremely reluctant even to recognize such a possibility. We have been too prone to take the cytoplasmic region of the cell-system, so to speak, at its face value; too ready to think of it as a vague and formless mass devoid of definite organization, or organized only by the domination of the nucleus. We have long been accustomed to think of the history of the cytosome as a simple mass division in fundamental contrast to the com-

plicated meristic process seen in nuclear division. Facts are, however, slowly accumulating which may compel a revision of this notion. Recent cytological studies bring prominently into view the fact that many of the formed bodies are directly transmitted either as such or in the form of pre-existing specific material from mother-cell to daughter-cell.

Vague indications of such a conclusion were long since given by the plastids of plant-cells, which are certainly in many cases, perhaps in all cases, self-perpetuating by growth and division without loss of their identity, though their distribution to the daughter-cells during division often seems to be an irregular or quite incidental process. At a later period it was demonstrated that the centrioles or central bodies, which form the foci of mitotic cell-division, are often self-perpetuating by a similar process, but in this case are handed on in a perfectly definite manner from cell to cell during division. More recent cytological studies raise the question whether still other formed bodies may not show a similar behavior. This question has not yet been definitely answered, but evidence has been coming in which places under suspicion the chondriosomes and the Golgi-bodies. Both these are in many cases handed on during division from mother-cell to daughter-cell, sometimes segregating with a precision that almost suggests that seen in case of the chromosomes. In many cases the chondriosomes take their place in separate groups about the equator of the karyolinetic spindle, draw apart into corresponding equal daughter-groups, move towards the poles and enter the respective daughter-cells. Whether these bodies individually considered have a permanent identity and are self-perpetuating by division is still a disputed question. Undoubtedly they are sometimes actually cut in two during cell-division; and in one well-determined case (spermatocytes of the scorpion *Centrurus*) all the chondriosomes become aggregated into a single ring-shaped body that is accurately divided in the course of the ensuing division. Often, it is true, the chondriosomes seem to be passively sorted out or segregated into two approximately equal groups; but in all these cases the possibility remains open that they may multiply by division at an earlier period. Were such the case their history in division would be comparable to that often seen in case of the plastids of plant-cells; and in point of fact an important group of observers, headed by Meves and by Guilliermond, have concluded from direct cytological observations that plastids may arise by the transformation of chondriosomes. If this should prove to be correct, substantial ground would be given for the conclusion that the chondriosomes may multiply by division since the plastids undoubtedly have this power. Still less is known of the Golgi-bodies in this regard; but recent studies have clearly shown that these bodies, too, group themselves in a definite manner about the mitotic spindle during cell-division and separate into two distinct groups which pass into the two respective daughter-cells.

Doubtful or disputed points aside, it already seems clear that in a large class of cases the specific substances of which the chondriosomes and Golgi-bodies are respectively composed are not formed *de novo* in the daughter-cells, but are somehow directly derived from corresponding components of the mother-cell. It is now certain, further, that in some cases their segregation in the daughter-cells can not be regarded as a merely passive or mechanical result of mitosis but is determined by a more definite and significant relation between these bodies and the centers of division; for as has recently been demonstrated by Bowen the chondriosomes are sometimes definitely oriented with respect to the centers in a manner that almost suggests that which characterizes the behavior of the chromosomes. In all this we see surface indications of a more deeply-lying process by which the complex cytoplasmic system may perpetuate itself intact from one generation to another or, by a modification of this process, may split up into secondary more limited systems according to a definite and predetermined plan. This will become clearer when we take a broader survey of the origin of the cytoplasmic formed bodies in general, a problem which now opens before us with a new significance.

It is possible that some, perhaps many, of the visible formed bodies are transmitted from mother-cell to daughter-cell in the form of chondriosomes, possibly also of Golgi-bodies, which are later transformed into bodies of

more differentiated types. An important group of observers have in fact advocated such a conclusion; but its validity still remains in doubt. On the other hand it is a widely prevalent view that many of the formed bodies arise *de novo,* being built up anew in the hyaloplasm by localized processes of chemical and morphological synthesis; but in respect to this question we may readily fall into error. Permit me to illustrate this by reference to some old observations of mine on those classical objects for the study of protoplasm, the transparent eggs of certain sea-urchins and starfishes.

When mature these eggs show with great beauty a structure somewhat like that of an emulsion, consisting of innumerable spheroidal bodies suspended in a clear continuous basis or hyaloplasm. These bodies are of two general orders of magnitude, namely, larger spheres or *macrosomes* rather closely crowded and fairly uniform in size, and much smaller *microsomes* irregularly scattered between the macrosomes, and among these are still smaller granules that graduate in size down to the limit of vision with any power we may employ. It is probable that both macrosomes and microsomes may be of several, perhaps many, different kinds; but this may here be disregarded.

The important fact here to be emphasized is that this so-called "alveolar" structure is not a primary characteristic of this protoplasm. It is of secondary origin, arising by the appearance in the homogeneous ground-substance of extremely minute scattered bodies which by growth and crowding together finally produce the emulsion-like structure. In the middle stages of this process the protoplasm gives an interesting picture. When viewed under a relatively low magnification, *e. g.,* 300-500 diameters, only the larger bodies are seen; but as step by step we increase the magnification, step by step we see smaller and smaller bodies coming into view, at every stage graduating down to the limit of vision. This remains true even with the highest available powers. The microscopical picture offered by such protoplasm is thus somewhat like the telescopic picture of the sky. At each step in the improvement of the telescope new and fainter stars have come into view. At each step the astronomer has felt sure that still more powerful telescopes would bring into view stars hitherto unseen. The cytologist is equally sure that if the present limits of direct microscopical vision could be extended we should see disperse bodies still more minute; and the invention of the ultra-microscope has in fact made us directly aware of the existence of suspended protoplasmic particles too small to be seen directly by the ordinary microscope, but made evident by their halos when viewed by the ultra-microscope in powerful reflected light.

In these eggs the smallest dispersed visible particles give us the impression that they are formed *de novo* in the structureless ground-substance. But manifestly it is illogical to affirm an origin *de novo* of any formed body because it first becomes visible at a particular enlargement, even the greatest at our present command. Here, clearly, is an enormous gap in our knowledge. All the available data—I can not here review them—indicate that below the horizon of our present high power microscopes there exists an invisible realm, peopled by a multitude of dispersed particles, a realm that is quite as complex as the visible one with which the cytologist is directly occupied. And the evidence further indicates, apart from all controversies concerning the nature of the so-called colloidal solutions, that many of these bodies are of much greater dimensions than the molecules of even the most complex organic substances.

We have now arrived at a borderland where the cytologist and the colloidal chemist are almost within hailing distance of each other —a region, it must be added, where both are treading on dangerous ground. Some of our friends seem disposed to think that the cytologist should here call a halt and hand over his inquiry to the chemist and the physicist with a farewell greeting. The cytologist views the matter somewhat differently. Unless he is afflicted with total paralysis of his cerebral protoplasm he can not stop at the artificial boundary set by the existing limit of microscopical vision. He is rudely pushed forward by the impact of a series of stubborn facts with which he must somehow try to reckon. He can not get out of his head that microscopical picture of progressively diminishing magnitudes which, as if viewed through an in-

verted telescope, disappear at last in vanishing perspective in the sub-microscopical depths. At the nearer end of this vista are the plastids, larger or smaller, each of its own specific type, and self-perpetuating by growth and division. A step beyond are the central bodies, often of such minuteness as to lie almost on the horizon of microscopical vision, but still capable of self-perpetuation by growth and division, of enlargement to form much larger bodies, and of exerting far-reaching effects on the surrounding structures. One more step and the cytologist is beyond the help of the microscope, wandering blindly in an unseen but none the less real world. The pathologists tantalize him with visions of disease-germs which no eye has yet seen, so minute as to pass through a fine filter, yet beyond a doubt self-perpetuating and of specific type. The geneticists continually crowd upon him with fresh demonstrations of those unseen somethings aligned in orderly array in the nuclear threads, each preserving its own specific type amid all the shifting events of the nuclear life, without modification by its fellows, and somehow, generation after generation handing on its individual characteristics to its descendants.

With all this in mind the cytologist finds reason enough to exercise his wits upon the apparently structureless ground-substance or hyaloplasm that seems to constitute the fundamental basis of protoplasm and to be the source of many of its formed elements. He can not resist the evidence that the appearance of a simple, homogeneous colloidal substance offered by the hyaloplasm is deceptive; that it is in reality a complex, heterogeneous or polyphasic system. He finds it difficult to escape the conclusion, therefore, that the visible and the invisible components of the protoplasmic system differ only in their size and degree of dispersion; that they belong to a single, continuous series, and that the visible structure of protoplasm may give us something like a rough magnified picture of the invisible. The cytologist is led still further to the conclusion that the ultra-microscopical dispersed particles of the hyaloplasm may be as highly diversified chemically as are the visible formed bodies, and that they are of all orders of magnitude; further, that it is they which constitute the sources, or at least the formative foci, of those larger formed bodies that we have so often but erroneously assumed to arise *de novo*. For my part, I am disposed to accept the probability that many of these particles, as if they were submicroscopical plastids, may have a persistent identity, perpetuating themselves by growth and multiplication without loss of their specific individual type. And lastly, there are many facts made known especially by experimental embryology, which indicate that it is in the apparently structureless hyaloplasm that the real problem of the cytoplasmic organization lies; and the same facts drive us to the conclusion that the submicroscopical components of the hyaloplasm are segregated and distributed according to an ordered system.

I am tempted to a larger development of this subject with reference to the problem of development, but time forbids. Good biological society has of late looked decidedly askance upon all corpuscular or micromeristic conceptions of the cell. To consider them seriously at this day requires a certain amount of courage. By some singular process of casuistry such conceptions have been supposed to place the fundamental problems of biology beyond the reach of scientific investigation. An ingenious philosopher has said that corpuscular hypotheses in general would make of the world—or of the cell—a mere puzzle-picture which we cut up into small pieces only to put them together again to form the same picture. The reply to this gibe, evidently, is the pragmatic one. Modern physical science has cut the whole world up into very small pieces and has thus far seemed to manage fairly well with the pictures rebuilt from them. The fathers of the cell-theory engaged in a somewhat similar operation when they resolved the living body into its component cells. Some of the successors of these pioneers, even down to our own day, have seemed to find something very reprehensible in this conduct; nevertheless, the cell-theory has somehow managed to survive as an effective means of biological progress. Perhaps, therefore, the youthful sciences of cytology and genetics may hope for lenient treatment if they try to go somewhat further along the path marked out by their forefathers. Many earlier hypoth-

eses of this type failed by reason of their too speculative character and because too much was claimed for them, either by their authors or by critics who wished to destroy them. Such was the case, for instance, with Weismann's speculations on the architecture of the germ-plasm, of DeVries on intracellular pangenesis, and of Altmann concerning the general significance of the protoplasmic granules. But we are not here concerned with merely theoretical constructions but with questions of fact that are directly and insistently forced on our attention by concrete microscopical and experimental studies on the cell. It is our business as students of cytology and genetics to answer these questions if we can. And lastly, I would remark that I am not here attempting to resuscitate the old conception of the cell as an assemblage or colony of elementary organisms or primary vital units —perhaps it is such, perhaps not—nor am I able to see how the possibilities here considered are in any manner out of harmony with the conception of the cell as a colloidal system.

We approach the final stage of our inquiry. We have, as it were (to return to Bergson's metaphor), taken the cell to pieces. How shall we put it together again? It is here that we first fairly face the real problem of the physical basis of life; and here lies the unsolved riddle. We try to disguise our ignorance concerning this problem with learned phrases. We are forever conjuring with the word "organization" as a name for the integrating and unifying principle in the vital processes; but which one of us is really able to translate this word into intelligible language? We say pedantically—and no doubt correctly—that the orderly operation of the cell results from a dynamic equilibrium in a polyphasic colloidal system. In our mechanistic treatment of the problem we commonly assume this operation to be somehow traceable to an original pattern or configuration of material particles in the system, as is the case with a machine. Most certainly conceptions of this type have given us an indispensable working method—it is the method which almost alone is responsible for the progress of modern biology—but the plain fact remains that there are still some of the most striking phenomena of life of which it has thus far failed to give us more than the most rudimentary understanding.

The nebulous state in which the whole concept of organization still remains is brought home to us when we attempt to deal with the fact that every organism either is, or at some time has been, a single cell. When it has come to full development the organism consists of coordinated parts, displaying a multitude of cunning devices—anatomical, physiological or chemical—that make provision for the harmonious cooperation of its activities and for its protection and maintenance. To this extent its organization is obvious and intelligible; and to the same extent the organism is clearly a piece of mechanism, a living machine. But let us review the building of this machine by following it backwards, step by step, to its starting point. Step by step we find the intricate machinery of life vanishing before our eyes until nothing remains but a single cell, the egg. In the egg-cell, complex though it may be in its own way, not a trace seems to remain of the coordinating and unifying devices of the adult; but who will maintain that the egg is not as specifically organized and as truly alive as the adult to which it gives rise?

It is an old notion to which modern research has given a certain semblance of support that the embryo is already present in the egg, blocked out, as it were, "in the rough" in the cytoplasm, so that development has only to impress upon it the finishing touches; but there is now conclusive evidence that the rough model, with the more than doubtful exception of one or two of its most general features, is itself the product of antecedent localizing operations of development. The main features of this process, often perfectly evident before the egg begins its cleavage into cells, may in some cases readily be followed by the eye. It is an impressive spectacle that is offered by the egg when busily engaged at its work of blocking out the embryo, without visible tools or model, but with an uncanny air of deliberate purpose and mastery of technique that any human artist might envy.

What then constitutes the organization of the egg? No one is yet able to answer. The embryologist, the cytologist, the physiologist and the biochemist—all of these alike have

thus far only skirted the outermost rim of the problem. We can not predict how far the cytologist of the future may be able to penetrate within it; but it would seem that sooner or later his way will finally be blocked by inherent limitations of the microscope determined by the wave-length of light. If we are ever to find our way into the innermost arcanum of the cell other methods must be employed; and we must marshal all the resources of experimental embryology, genetics, biophysics and biochemistry. Experimental embryology has contributed many important discoveries towards elucidating the phenomena of development, but it has also emphasized our failure thus far to solve the central problem. From this source, indeed, came the facts on which Driesch, a distinguished pioneer in this field, based his famous argument against the machine-theory of development and in favor of a new philosophy of vitalism. The rock on which the whole mechanistic conception of organization and development splits, he insisted, is the fact that a *fragment* of an egg may undergo complete development and produce a perfect dwarf embryo. This argument may fail to convince us—it does fail—but no one has yet found an adequate reply to it. All, on the contrary, now points to the essential correctness of Driesch's contention that at the real beginning of development the cytoplasm of the egg is devoid of any structural pattern or machine-like configuration that foreshadows the plan of the future embryo. Not alone the structural details of the embryo but the very plan on which it is built is constructed anew in the course of development.

May we then seek a solution of the puzzle in the nucleus of the egg? Perhaps. It is no longer open to doubt that the development of particular characters somehow depends upon the presence in the nucleus of corresponding particular and separate units; and this conclusion loses nothing of its force by reason of the fact that the precise nature of the units is still unknown. We know from Boveri's celebrated experiments that normal development depends on the normal combination of these units. Genetic evidence is now opening far-reaching horizons of future discovery by the accumulating demonstration that no one of the nuclear units plays an exclusive rôle in the determination of any single character. It has been made clear that the individual unit may affect the production not merely of one character but of many. Conversely the probability is shaping itself that the production of any single character requires the cooperation of several or many units, possibly of all. I believe it is not a great overstatement when I say that every unit may affect the whole organism, and that all the units may affect each character. We begin to see more clearly that the whole cell-system may be involved in the production of every character. How then are hereditary traits woven together in a typical order of space and time? It is the same old puzzle made larger and more insistent but not yet, so far as I can see, brought nearer to its solution. We are ready with the time-honored replies: It is the "organism as a whole"; it is a "property of the system as such"; it is "organization." These words, like those of Goldsmith's country parson, are "of learned length and thundering sound." Once more, in the plain speech of everyday life, their meaning is: *We do not know.*

I do not in the least mean by this that our faith in mechanistic methods and conceptions is shaken. It is by following precisely these methods and conceptions that observation and experiment are every day enlarging our knowledge of colloidal systems, lifeless and living. Who will set a limit to their future progress? But I am not speaking of to-morrow but of to-day; and the mechanist should not deceive himself in regard to the magnitude of the task that still lies before him. Perhaps, indeed, a day may come (and here I use the words of Professor Troland) when we may be able "to show how in accordance with recognized principles of physics a complex of specific, autocatalytic, colloidal particles in the germ-cell can engineer the construction of a vertebrate organism"; but assuredly that day is not yet within sight of our most powerful telescopes. Shall we then join hands with the neo-vitalists in referring the unifying and regulatory principle to the operation of an unknown power, a directive force, an archæus, an entelechy or a soul? Yes, if we are ready to abandon the problem and have done with it once for all. No, a thousand times, if we hope really to advance our understanding of the living or-

ganism. To say *ignoramus* does not mean that we must also say *ignorabimus*. I do not believe that a confession of ignorance leaves us with no recourse save vitalism. To maintain that observation and experiment will not bring nearer to a solution of the puzzle would be to lapse into the dark ages. Perhaps Professor Henderson is right when he expresses his belief that organization has finally become a category that stands beside those of matter and of energy. Perhaps there is no problem or none that we can formulate without talking nonsense. Perhaps we should go no further than to record and analyze the existing order of phenomena in living systems without losing sleep over the imaginary problem of a unifying principle. Let us politely salute all these uncomfortable possibilities and go our way. For my part, I find it more amusing to look forward to a day when the great riddle may give up its secret.

EDMUND B. WILSON

DARWIN AND PASTEUR: AN ESSAY IN COMPARATIVE BIOGRAPHY[1]

PLUTARCH'S "Parallel Lives," although read and admired throughout the ages, have found remarkably few imitators. Why this is so would be an interesting question. Perhaps the rise of Christianity, with quintessence of altruism and an instinctive recognition that comparisons, if not necessarily odious, are often unkind, has something to do with it. Perhaps the spirit embodied in that most charitable of pagan maxims—*de mortuis nil nisi bonum*—has also played its part. The modern neglect of Plutarch's method is the more remarkable because it is the basic method of modern science, and the tap root of modern thinking and working. Science owes an immense and growing debt to comparative anatomy, comparative geology, comparative physiology, and comparative pathology. More lately it has produced also the rich fruits of comparative philology, comparative philosophy, comparative politics, comparative psychology, comparative religion and comparative literature. Why not, then, back to Plutarch and the potential field of comparative biography?

Ruminating thus in the blessed quiet hours of a professorial holiday, and braced by the cool airs of an Alpine valley, I could not resist the temptation to revive Plutarch's method by a comparison of the two great master minds of the Victorian era whose labors have thrown upon the mysteries of the living world a clear and penetrating light, a blaze which time may dim but can never extinguish.

Charles Robert Darwin wrought upon the mind of his time a complete change in the point of view concerning the origin, the nature and the relationships of mankind and other living things. Louis Pasteur disclosed to the astonished gaze of the nineteenth century a new world of microscopic life dwelling upon us, within us, and about us, working sometimes for good and sometimes for evil. Darwin was a silent Savonarola, Pasteur a sedentary Columbus of Biology. Such masters invite study and comparison.

To Charles Darwin and Louis Pasteur belongs the rare distinction of having changed completely the point of view of their own and probably later generations.

Nothing had been more interesting or more puzzling throughout the ages than the origin and relationships of the various kinds or species of plants and animals. Dogs, cats and sheep; oaks, elms and willows—how did they come to be so alike, and yet so different? The ancients had their theories, but these were set aside or forgotten in the Christian world when the biblical account of creation came to be literally accepted. That account, like many that had preceded it, affirmed a strictly supernatural origin for plants and animals, and so overcame all difficulties. But by the middle of the nineteenth century the world was growing impatient of supernaturalism, especially in the exaggerated form this had taken on in the Middle Ages, and was ready for a change, so that when Darwin published his great work on the "Origin of Species" in 1859 it was com-

[1] The accompanying essay was left uncompleted by the late William Thompson Sedgwick of the Massachusetts Institute of Technology, when he died, as we all wish to, quickly and before his work was finished. Such an essay may be not only especially timely in this Pasteur anniversary, but may also be useful at a time when men of faith are attacked by men of ignorance and credulity.—G. J. P.

paratively easy for him to convince his contemporaries that the various kinds of plants and animals (including man) have come into existence gradually and naturally, rather than suddenly and supernaturally. This he did by powerful arguments supported by long chains of evidence for the transmutation of species mainly under the influence of a new principle or natural law which he called natural selection, and Herbert Spencer named the survival of the fittest. The simple supernatural theory had long failed to satisfy the minds of many thoughtful men, but, since these could not discover any other or more reasonable explanation of the great mystery, they either adopted the waiting attitude of agnosticism or else acquiesced nominally and perfunctorily in the current hypothesis. Meanwhile, a way had been paved for the easier acceptance of the Darwinian theory of gradual transmutation by the geologists, some of whom were now teaching that the various features of the earth had come into being slowly and naturally rather than suddenly and supernaturally, and by the archeologists who were finding almost daily evidences of the great antiquity of man and of his low condition in antiquity.

Hardly less interesting to mankind than his own origin and the origin of other forms of life have been, throughout the ages, the origins of plague, pestilence and sudden death. Those fearful phenomena, however, seemed to our more thoughtful forefathers hardly more mysterious than did swift decay or riotous putrefaction, while the fermentation of wine and beer and bread had long puzzled the wisest. Here, too, in ancient and medieval times, supernatural explanations were invoked, and here also for those who could not entertain such explanations, and for almost everybody by the middle of the nineteenth century, the waiting attitude of agnosticism had come to be the only rational attitude. Almost at the very moment when Darwin startled the world by the publication of the "Origin of Species," that other seer of the nineteenth century, Louis Pasteur, was solving the problems of fermentation, putrefaction and decay which, as he soon discovered, led him straight into the tangled mysteries of contagious and infectious diseases.

Happily, we possess of these two men full and intimate biographies, Darwin's by a son, and Pasteur's by a son-in-law. For Darwin we have also a brief but precious autobiographic sketch, and it is understood that M. Vallery-Radot must have had Pasteur's assistance in his work. None who wishes to feel for himself the spirit of nineteenth century science should fail to peruse these two great biographies. And if, along with these, or after them, he turns to that of Thomas Huxley by his son Leonard, he will be at once instructed, entertained and amused by the life-history of Darwin's great apostle, one of the bravest, brightest, honestest and wittiest of Victorian Englishmen.

Darwin and Pasteur were for sixty years contemporaries, but Pasteur was the younger by thirteen years. Darwin was born in February, 1809, Pasteur in December, 1822. Born thirteen years apart, they died thirteen years apart, both at the age of seventy-four years. The two youths, as things go to-day, were near neighbors, for Paris and London are not now far apart. But when they were boys or young men this was not the case. Napoleon, whose great deeds formed—so Pasteur's biographer tells us—a kind of historical background of glory for the Pasteur family, was at the same time probably thought of with disgust in the Darwin household. France and England had long been hereditary enemies rather than friendly neighbors, and the two boys, though so near, grew up with very different ideals and under different conditions. Both had good parents and good homes, but Darwin's parents were gentlefolk, while Pasteur's were hardworking tradespeople. Darwin's father was a well-to-do practising physician, Pasteur's an impecunious tanner. Darwin's grandfather, Erasmus Darwin, had a strong taste for science, coupled with an imagination which led him to write verses entitled "The botanic garden." Pasteur's father was a veteran of the Napoleonic wars, proud of his loyal service. His grandfather was an unsuccessful tanner. Darwin's mother was Susannah Wedgwood, daughter of Josiah Wedgwood, the successful and famous manufacturer of pottery. Pasteur's mother was Jeanne Roqui, the daughter of a gardener. Through her mother the Pasteurs later inherited some small sum. Pasteur's mother was "very active,

full of imagination" and enthusiasm. Of Darwin's mother, who died when he was only eight years old, we know but little, but her grandson says "we may hazard the guess that Charles Darwin inherited from the Wedgwood side his sweetness of disposition, while the character of his genius came rather from the Darwin grandfather."

The schooling of the boys was different. For Darwin it was the regular training of well-to-do English boys, *viz.*, a public school; for Pasteur, the corresponding French training for a poorer boy. For the Darwin family it was much a matter of course; for the Pasteur family a hard struggle nobly conceived and bravely executed. The boys themselves seem to have been not unlike other boys; not very studious, not very appreciative of their opportunities. At that time, no one, probably, would have predicted for either any great distinction above his fellows.

As to religious training we are told but little for either, but we get the impression that while Pasteur was surrounded by an atmosphere of strong Roman Catholic piety, Darwin had the ordinary and conventional religious training of a Church of England family.

In his nineteenth year Darwin is uncertain of his career, giving his father anxiety, and shifting from would-be physician to clergyman. He goes to Cambridge and, though to be a clergyman, drifts into a sporting set which sometimes drinks too much. No word is ever said about the want of money. Pasteur in his nineteenth year has just taken his *bachelier ès lettres* and has become a kind of sub-master in the college of Besançon. He was by this time in deadly earnest—too earnest. Unselfishly and bravely he offers to pay for his sister's schooling, though earning only 24 francs a month.

At 22, when American boys leave college, Darwin begins his real life work—engaging to go on the *Beagle* and giving up medicine and divinity. At 22 Pasteur is in training at the Ecole Normale and teaching his old father science.

At 30, Pasteur is three years married and deep in his researches on the tartrates. At 30, Darwin marries and writes up his *Beagle* Journal, a geologist and recognized as a good man of science but hardly as yet for original work.

At 40 (1849) Darwin is engaged in "the slow journey towards the origin of species." At 40 (1862) Pasteur had studied fermentation of beer and vinegar. Was elected to the Academy and advised by Balard and Dumas to give up crystallography and prosecute his studies of ferments.

At 50 (1859) Darwin published his "Origin of Species." At 50 (1872) Pasteur, like Luther, propounded his theses in the fire of controversy.

At 60 (1869) Darwin is now fighting, too. At 60 (1882) Pasteur is triumphant.

At 70 (1879) Darwin is practically triumphant. At 70 (1892) Pastur is completely triumphant.

Pasteur, more intense, impatient, more enthusiastic, more conscientious, became serious earlier, achieved earlier, married earlier, won distinction earlier, aroused controversy earlier, triumphed earlier, but did not die earlier. Pasteur had better health (the first wealth is health) though once almost killed by overwork.

For both the non-effect of environment.

For both industry, concentration, struggle.

For both natural law in place of mystery.

For Pasteur, minutæ, laboratory, precision.

For Darwin, broad principles, masses of evidence, field work, less laboratory.

For Pasteur, "the infinitely little."

For Darwin, the continuing development of mankind.

Both Darwin and Pasteur were fortunate in bringing out their great ideas at the right time. (Agnosticism in medicine, agnosticism in cosmology). The world was tired of supernaturalism and ready for naturalism. It was tired of confusion and ignorance concerning disease, and eagerly embraced the germ theory.

The fame of Darwin has grown greater with the passing years. Darwinism has already become merged and may one day become submerged in the broader doctrine of evolution, of which it was the forerunner. Pasteur's name, curiously enough, is popularly best known in pasteurization, a process of applied science employed long before his day under other names and no name, but first made rational and scientific by him. But Pasteur's original ideas

and discoveries have spread like an infection until to-day they cover the earth.

Darwin, the master of the organic world, sleeps near Newton, the master of the inorganic, in the great Abbey, among the most famous of his race. Pasteur rests alone in the chapel of his laboratory. The world claimed Darwin's body to place among its great ones. Science kept Pasteur's for its own. Both dwell forever among the immortals. The last half of the nineteenth century may well be called their age—the Age of Darwin and Pasteur.

EDWARD EMERSON BARNARD

THE death of E. E. Barnard on February 6, 1923, brought to a close a life and a career which were at once among the most notable and the most inspiring recorded in the annals of American science. The loss will be felt almost as keenly in every country where science is cultivated and loved. Here and there in every generation there breaks through the magma of our common clay a man whose mind is lighted and whose will is energized by a ray of genius. Such a man was he. The manner in which Nature chooses her sparking points and brings together the unusual elements of personality which make a marked man is only beginning to be recognized.

The outward circumstances of Mr. Barnard's life are so well known to most readers of SCIENCE that recitation of them is barely necessary. Even to himself as he struggled and won there must have come often the thrill of romance. To others, unacquainted with the details of his heroic struggle, he personifies in his life the freedom, the opportunity, the vigor and love of action inherent in our great democracy. Sketches and books will be written about him. No better investment in personality could be made than to render the facts and circumstances of his life and work easily accessible to the boys and girls of our country, whom he loved so well and believed in so fervently.

Mr. Barnard was born in Nashville, Tennessee, December 16, 1857, and died in his home near the Yerkes Observatory in the evening of February 6, 1923. His father died before he was born and his mother in 1884. One brother survives him. His early education was given him by his mother. His work as a boy helper in the photographic studio of the brothers Calvert in Nashville and the loan of Dr. Thomas Dick's book, "The Practical Astronomer," combined to stir his imagination and direct his attention toward the subject which was to become his lifelong and sole passion, astronomy. The acquisition of a five-inch telescope in 1877 led him on in his study of the celestial objects, notably Jupiter. In this year the annual meeting of the American Association for the Advancement of Science was held in Nashville and he became a member. On January 27, 1881, he married Miss Rhoda Calvert, the gentle sister of his employers, whose life became devoted most completely to solicitous care for her husband and whose death came less than two years before his own.

On the morning of May 12, 1881, he found a faint comet in the field with Alpha Pegasi and after observing it again the next morning he telegraphed the discovery to Mr. Lewis Swift, of Rochester, N. Y., a veteran comet-seeker. In spite of diligent search by both of these men, and by others, no further trace of it was ever found. Either just before or after this experience Mr. Barnard began his systematic search for comets and on September 17 he was rewarded by finding the comet which is known as Comet VI 1881. Before he left Nashville to join the newly organized staff of the Lick Observatory on Mount Hamilton in California he had discovered nine new comets. He later found seven more, bringing his total up to sixteen. One of these he found by photography on October 12, 1892, the first ever found in this manner.

His early discoveries and his earnestness and zeal attracted the attention of the chancellor and the instructors of the recently established Vanderbilt University at Nashville. In 1883 he was offered a fellowship at Vanderbilt and was given such sympathetic assistance that by tutoring and studying day and night he was able to graduate in 1887. During this time he was in charge of the university's six-inch telescope. In 1889 he received the A.M. degree of the University of the Pacific, in 1893 the degree of Sc.D. from his alma mater and in 1909 the degree of LL.D. from Queen's University, Kingston.

At the Lick Observatory his association with Sherburne Wesley Burnham cemented a friendship which lasted for life. Mr. Burn-

ham, his senior by nineteen years, was already at that time a famous observer whose kindly interest was reciprocated by the greatest admiration on the part of the younger man. When the Yerkes Observatory of the University of Chicago was organized in the middle nineties, both became professors of practical astronomy on its staff and remained so, Mr. Burnham until he retired in 1914 at the age of seventy-five and Mr. Barnard until his death.

While at the Lick Observatory he continued his visual work on comets and the planets and developed the methods of photographing comets and the star clouds and nebulosities of the Milky Way. Then, on September 9, 1892, he glimpsed the diminutive inner moon of Jupiter, with the thirty-six inch refractor, confirmed his observation on the following nights and in a few weeks he had convinced an incredulous world of astronomers that Jupiter has a fifth satellite which is unique in its location and behavior as well as most difficult to see on account of its close proximity to its brilliant master.

Almost immediately, recognition for this remarkable discovery was given him, when, before the year was out, the French Academy of Sciences voted him the Lalande Medal, following up its growing admiration of his feat by giving him the Arago Medal in 1893. In 1897, formal recognition of this discovery and of his other work came from the Royal Astronomical Society of London in the shape of its gold medal. Later, in 1900, the French Academy again honored him with the Janssen Gold Medal. Then, in 1906, the Astronomical Society of France presented him its Janssen Medal and in 1917 the Bruce Gold Medal of the Astronomical Society of the Pacific was awarded him for his long and notable activity. Only the year before, he had added to his list of discoveries the faint star in Ophiuchus, which has the largest known proper motion, 10.3 seconds of arc a year, and a parallax exceeding half a second of arc. He was a member of the National Academy, the American Philosophical Society and the American Academy of Arts and Letters. Since 1914 he was a trustee of the Benjamin Apthorp Gould Fund and an associate editor of the *Astronomical Journal*.

Like his great friend Burnham a tireless worker, he was most unlike him in the distribution of his efforts. Mr. Burnham concentrated on two allied lines of work, orbital motions and proper motions of stars. Mr. Barnard's published observations encompass every type of object in the sky. While thoroughly philosophic in his method and in his attitude of mind his interest was in discovery and description rather than in theory. His published articles and notes number more than eight hundred and forty. Here, to mention a few, are announcements of comets; positions, photographs and drawings of these fascinating objects, which never ceased to thrill him, and of surface details of the planets; measures of sizes and positions of primaries and satellites; exhaustive light curves of novæ and of variable stars, for one of which, at least, he gave the period to such a degree of accuracy that he was wont to say it could be used as a fundamental clock, in somewhat the same manner that Mr. Michelson's wave-length of the red line of cadmium can be used as the standard of measurement of length; long lists of auroræ with dates and descriptions; discussions of meteoric trails and methods of defining them precisely; discussions of the technique of photography and the performance of lenses; measures of difficult and important double stars like Sirius and Procyon; the parallax of Krüger 60 from micrometric measures; a long series of micrometric measures of position of Eros in 1900-1901; magnificent photographs of terrestrial cloud forms which have found their way into treatises on the atmosphere of the earth; a masterful biographical appreciation of his friend Burnham; photographs and description of the solar corona obtained in the three total eclipses which he observed, in California, 1889, North Carolina, 1900, and Wyoming, 1918;[1] observations of the Zodiacal Light, the Gegenschein, which he discovered independently in 1883, and the luminous night haze. Volume XI of the Publications of the Lick Observatory is made up of his photographs of the Milky Way and of comets taken at Lick and his notes and descriptions of them.

[1] All his preparations and the longest journey he ever made to observe a total eclipse of the sun were in vain when bad weather obscured the sun at the critical time in the Sumatra eclipse of 1901.

Up to within less than a week of his death, while in bed, he worked with his invaluable assistant, Miss Mary Ross Calvert, upon the text of his "Atlas of the Milky Way" from negatives taken at the Yerkes Observatory and, for some of the more southerly fields, at Mount Wilson during a stay of some months at that observatory in 1905. This atlas, which he had hoped to finish during the present year, will be published by the Carnegie Institution of Washington. His close study of these magnificent photographs led him to distinguish sharply between what he held were vacant spaces in the galaxy and those due to opaque masses of matter lying on the near side of the star clouds. His great series of triangulations with the micrometer on many of the globular clusters were continued for a quarter of a century and will be published in due time. When he began them he expected to find internal motions within a few years, but, as he measured, his universe grew larger and even with the great Yerkes telescope he was able to find the slightest traces of motion only for a very few stars. His one hobby was his orchard, where he fought valiantly and scientifically the pests which would rob him of his fruit. His eye was keenly open to terrestrial nature about him and he never wearied in telling the beauty of the swelling bud, the marvelous structure of the oak ball, the mysterious career of the *Cicada septendicem* or the adaptation of form to mode of life of the lowly chigger.

Observing with a large telescope was to him as a sacrament and with his highly varied program of observation he would persist under atmospheric conditions which compelled other observers to desist. When he lectured, as he did often and well, he was not at ease until he became so completely engulfed in the flow of his presentation that his self-consciousness left him.

More than most men, he was beloved by his fellows for his unselfish charm. We, who have lived with him for many years, miss his light footfall in the corridor, his friendly smile and his charitable counsel.

Astronomy, which gave him his greatest pleasure in life, was herself immeasurably enriched by his restless activity for nearly half a century.

OLIVER J. LEE

SCIENTIFIC EVENTS

THE HABER PROCESS[1]

THE lecture delivered by Professor F. Haber on the award of the Nobel Prize at Stockholm on June 1, 1920, is printed in *Die Naturwissenschaften* for December 8. Professor Haber dealt first with the work done on the synthesis of ammonia before his first research in 1905. Practically nothing of importance had come to light, and the very small yields at ordinary pressures did not hold out much promise of technical application.

The early experiments of Haber, like most of those which have served as the foundations of great industrial undertakings, were made with a purely scientific object, and with no technical applications in view. The results obtained, however, soon made it clear that the basis of an important technical process could be found in ammonia synthesis, and further work was undertaken with this end in sight.

In 1908 the Badische Gesellschaft placed at Haber's disposal all the means requisite for the further progress of the research on the synthesis of nitric oxide in the electric arc which he had begun in 1907, but his proposal to undertake research on the synthesis of ammonia was received with open doubts as to the potential value of the method. The nitric oxide syntheses, in cooled arcs under reduced pressure, and in flames and explosions, were not found suitable for technical application, and attention was then turned to the stone which the builders had rejected. The judgment of the technical chemists of the Badische Gesellschaft had been at fault, since ammonia synthesis was ultimately a very real solution of the problem of the economic utilization of atmospheric nitrogen.

Ramsay and Young in 1884 had found that with nitrogen and hydrogen in presence of iron at 800° C. no ammonia was produced. This was found to be incorrect, and traces of ammonia were detected. Other catalysts were tried, and from the results it was evident that an equilibrium state was attained, from which it was possible to calculate the yields at other temperatures and pressures. No further progress was made, however, since it was judged by the technical experts to be impos-

[1] From *Nature*.

sible to carry out the reaction on the large scale at the temperatures required under the very high pressures indicated by the calculations.

In 1906 measurements under pressure were for the first time carried out by Nernst and Jellinek (these are not referred to by Haber), and in 1908 Haber in conjunction with Dr. Le Rossignol began experiments at higher pressures. The work of Le Rossignol (a British subject) is spoken of with great approbation, although his part in the achievement of success has perhaps not always received full credit in some quarters. The technical chemists were still unfavorably inclined towards the process, although practical yields had now been reached. By the use of new catalysts the temperature was lowered to 500-600° under a pressure of 200 atmospheres. In 1913 the process was taken up by the Badische Gesellschaft, but an account of the main scientific results was also published. The work of Dr. Bosch speedily led to the successful introduction of the synthetic ammonia process, and in the period 1913-1920 the capacities of the German factories rose from *nil* to 35,000 tons per annum in 1914, 850,000 tons in 1918 and 1,500,000 tons in 1920.

INTERNATIONAL SCIENTIFIC COOPERATION

THREE committees appointed by the Commission on International Intellectual Cooperation of the League of Nations held sessions in Paris at the end of December, namely, the committee on bibliography, the committee on matters pertaining to universities and the committee delegated to study into the question of intellectual ownership.

A report in the *Journal* of the American Medical Association states that the committee on bibliography is engaged in the coordination of bibliographic enterprises undertaken in the various countries, with a view to avoiding duplication of effort and assuring to scientists and investigators the advantages of a complete bibliography easily accessible. The committee on university matters is dealing with various questions relative to the organization of the international congress of universities. The committee, however, recognizes the fact that political conditions will not permit the convening of such a congress at present. But it is possible to take up at once an extended inquiry into the best means of bringing about a cooperation between the universities. Opportunity is offered also for the discussion of kindred questions, such as the publication of an annual catalogue containing a list of all the courses given in the universities of the world, and the creation of a bureau of information pertaining to universities.

The committee on intellectual ownership is studying more particularly the question of the extension of authors' rights in the field of science, so as to secure to the individual scientist and the field of science represented by his country a more adequate participation in the benefits arising from the utilization of his discovery. At present the system of patents protects only the inventor of the industrial application of a scientific discovery. The mathematician, the physician, the chemist and the biologist, whose scientific discoveries made possible the invention, derive no benefit either for themselves or for their laboratories from the utilization of their works. At the instance of the chairman, M. Bergson, the committee delegated M. Ruffini, professor in the University of Turin, to draw up a plan which, subject to the approval of the commission on intellectual cooperation and the council of the League of Nations, might be used to establish the juridical principles for international protection of scientific discoveries. Such a plan might serve later as the basis for drafts of laws which, with the necessary modifications, might be presented to the parliaments of the various countries. With this object in view, M. de Torrès y Quevedo, of Madrid, was designated to study the possibilities of applying, in a practical way, the juridical principles and the suggestions for legislation contained in the document to be drawn up by Ruffini.

RESEARCH SERVICE TO THE INDUSTRIES OF MICHIGAN

UNIVERSITY extension service to the manufacturing and technical interests of the state is being introduced to the Michigan manufacturer through the issuing and distribution of a booklet explaining the purpose and facilities of this special department.

Professor A. E. White, director of this de-

partment, has been investigating the problems of the state's industries and his purpose is to put the facilities of the various university laboratories at the service of Michigan manufacturers. Through this service trained specialists in practically all phases of research work will be available to the manufacturer. The large and complete technical library of the university will also be at the disposal of any one having special problems.

It is believed that there is a definite need for a research service of this kind, and that many Michigan industries will avail themselves of the service as in many cases the type of research which is necessary may be conducted only with expensive equipment such as is contained in many of the university's laboratories.

The types of service of this department are of three kinds. The library service, in addition to making available the scientific library, is prepared to make photostat prints, write translations and do other work of a like nature. The second division of the service is for the study of problems requiring an ultimate though not immediate answer. The third is for the investigation of problems which require the full time service of one or more persons for a period of six months or longer. This includes the chemical and physical tests.

The problems which may be studied through the department with the present facilities are grouped under the following specific types of service: aeronautics, automotive equipment, ceramics, design of special machinery, electric transmission and distribution, fuels, heating, ventilating and illumination, hydraulics, machine shop practice, power plants and their equipment, radio and telephony and tests of building and engineering materials.

PUBLIC LECTURES OF THE CHICAGO SECTION OF THE AMERICAN CHEMICAL SOCIETY

THE Chicago Section has launched an aggressive campaign to popularize chemistry by choosing speakers, and by advising various civic, social and cultural organizations in the city that these men and women are willing to talk to them about chemistry. Two letters have been sent out; the first to speakers, advising them as to the best methods of making popular addresses, and the second to organizations which the section believes will entertain the idea of learning some of the modern facts about chemistry. Below is the list of speakers chosen for this work:

G. L. Wendt, Ph.D., chief research chemist, Standard Oil Company of Indiana; former professor of chemistry, University of Chicago.

W. Lee Travis, Ph.D., head of the chemistry department of Northwestern University; developer of war gas known as "Lewisite."

David Klein, Ph.D., technical director at the laboratories of Wilson and Co.; formerly assistant professor at the University of Wisconsin; state chemist of Illinois; specialist in the use of internal glandular extracts in medicine.

Otto Eisenschiml, head of the Scientific Oil Compounding Company: "Chemistry as a career."

Julius Stieglitz, Ph.D., head of department of chemistry of the University of Chicago: "Chemistry in everyday life."

Mrs. L. F. Supple, assistant professor of textile chemistry at Lewis Institute: "The chemistry of cloth."

P. N. Leech, Ph.D., laboratories of the American Medical Association: "Home remedies—their use and abuse."

G. A. Menge, Ph.D., consulting chemist: "Milk—its various available forms and their relative merits."

H. E. Barnard, Ph.D., director American Bakers Institute; formerly state food commissioner of Indiana: "The bread of life—its rôle in nutrition."

I. K. Russell, editor of *Baking Technology;* formerly on editorial staff of the New York *Evening Mail:* "Pasteur, the chemist."

F. L. Dunlap, Sc.D., consulting chemist; formerly associate chief, United States Bureau of Chemistry: "What do we know about flour?"

C. S. Miner, consulting chemist; head of the Miner Laboratories: "The chemist in the courts."

W. D. Harkins, Ph.D., professor of chemistry, University of Chicago: (1) "Radioactivity"; (2) "The building of atoms."

J. A. Hynes, chief chemist of the United States Customs Service at Chicago: "Reminiscences of a customs house chemist."

G. W. Hoover, M.D., chief, Chicago Station, United States Bureau of Chemistry: "Food and drug frauds."

E. H. Volwiler, Ph.D., chief chemist, Abbott Laboratories: "Coal tar in medical science."

W. D. Richardson, chief chemist, Swift and Company: "Vitamines in everyday diet."

L. M. Tolman, chief chemist, Wilson and Company: "Shortening fats and household economy."

J. J. Vollertson, chief chemist, Morris and Company: "The chemist in the packing house."

William Hoskins, consulting chemist; dean of Chicago Chemists; head of Mariner and Hoskins: "Reminiscences of a consulting chemist; snow crystals."

R. E. Doolittle, chief, Central District, United States Bureau of Chemistry: "Food Poisoning."

Katharine Blunt, Ph.D., associate professor, home economics, University of Chicago: "Chemistry in domestic science."

William D. McNally, chief chemist, coroner's laboratory, Chicago: "Chemistry in the coroner's office."

H. N. McCoy, Ph.D., director, Lindsay Light Company; formerly associate professor of chemistry, University of Chicago: "Development of the electric light."

F. C. Whitmore, Ph.D., professor of organic chemistry, Northwestern University: "Organic chemistry and public health."

J. R. Chittick, Pharm.D., chief chemist, Jaques Manufacturing Company; formerly associate professor of chemistry, Highland Park College, Iowa: "Chemistry of baking powders."

R. C. Coleman, chief chemist and general manager Economy Fuse Company: "Chemistry of paints and varnishes."

Max Henius, Ph.D., formerly co-head of the Wahl-Henius Institute: "Chemistry of fermentation."

ORGANIZATION OF SCIENTIFIC MEN IN THE NORTHWEST

IN Spokane a meeting of people interested in the advancement of science was held at a banquet at the Davenport Hotel, on the evening of February 9. Thirty-six sat at the table. Representatives from the science faculties at Washington State College, University of Idaho, State Normal School at Cheney, Whitworth College, Spokane University, North Central High School and the Lewis and Clark High School were present besides a number of others. Mr. L. C. Armstrong, secretary of the Columbia Section of the American Institute of Mining and Metallurgical Engineers, presided. Discussion brought out clearly the need of an organization with a meeting center in the northwest. Only nine people from the four northwest states out of an American Association for the Advancement of Science membership of about three hundred attended the Boston meeting. Letters were received from President Evermann and Secretary Sargeant, of the Pacific Division, and Secretary Livingston, of the American Association, as also from Chancellor Brannon and President Clapp, of the University of Montana; President Holland, of Washington State College, and Professor Brode, of Whitman College, who were unable to attend. A committee consisting of Dr. M. A. Angell, of Idaho University; Dr. Frederick Heald, of Washington State College; Dr. Geo. W. Hess, of Whitworth College; Miss Elizabeth L. Martin, of Cheney State Normal School; Superintendent O. C. Pratt, Spokane public schools, and Mr. Frederick Keffer, mining engineer, were appointed to draw up plans for a permanent organization to be reported at the time of the meeting of the Inland Empire Education Association in Spokane the first week in April. Letters have been sent out by the chairman and secretary of the preliminary meeting to a large number of scientists residing in the territory of the "Inland Empire Association" advising them of this committee and asking them to communicate their ideas to the committee on the type of organization which would best serve the interests of this region. Two types were discussed at the meeting: first, a division of the American Association or a section of the Pacific Division; second, an organization of the familiar academy of science type which would furnish a means of publication of matter of a regional nature. An organization combining both features was suggested by Secretary Livingston.

THOMAS LARGE,
Secretary

NEW HAVEN MEETING OF THE AMERICAN CHEMICAL SOCIETY

THE sixty-fifth general meeting of the American Chemical Society will be held at New Haven from April 2 to 7.

A symposium on "Motor fuels" will be held jointly by the Division of Petroleum Chemistry and the Section of Gas and Fuel Chemistry. The results of various investigations as to the worth and quantity obtainable of alcohol blends, new methods of producing gasoline from petroleum and natural gas, work in blends of benzol and kerosene and other blends will be reported. Papers will be presented also covering discoveries in regard to anti-

knock compounds, more efficient carburetion and other subjects which have a direct bearing on the problem of keeping the supply of motor fuels of all kinds equal to the demand.

The Division of Dye Chemistry has arranged for a symposium on "The coal tar dye industry in the United States prior to 1914." Nutrition will be discussed in the meeting of the Division of Biological Chemistry, which will hold a symposium on that subject. Attempts to isolate and get a better understanding of vitamines will be reviewed. Agricultural chemists have arranged a symposium on "Insecticides and fungicides" at the meeting of the Division of Agricultural and Food Chemistry.

Chemical problems involved in the production of artificial silk paper, gun cotton, smokeless powder, celluloid and pyroxylin lacquers will be discussed in various papers to be presented before the Division of Cellulose Chemistry. This division will deal particularly with recent achievements in the artificial silk industry at a symposium on "Oxycellulose, cellulose hydrate and hydrocellulose." The Division of Rubber Chemistry will honor the memory of Charles Goodyear, who, by his pioneer work on the vulcanization of rubber, made possible the rubber industry to-day.

Among those who have accepted invitations to speak at the general sessions of the society are Secretary of War John W. Weeks and Dr. Francis P. Garvan, president of the Chemical Foundation.

The dedication of the Sterling Chemistry Laboratory, of Yale University, will be held on Wednesday, April 4, at which members of the chemical society will be guests of the university. Chemists of international note who will be present and take part in the dedication exercises and also in the chemical society meetings will be Professor W. Lash Miller, of Canada; Professor G. Urbain, of France; Professor F. G. Donnan and Principal J. C. Irvine, of England; Professor G. Bruni, of Italy; Professor A. F. Holloman, of the Netherlands, and Professor The. Svedberg, of Scandinavia. Professors M. Gomberg, G. N. Lewis and A. A. Noyes, among others, will represent the United States.

SCIENTIFIC NOTES AND NEWS

DR. EDOUARD BENJAMIN BAILLAUD, director of the Paris Observatory, has been given the Bruce gold medal, awarded by the Astronomical Society of the Pacific. The presentation was made on February 27 by Myron T. Herrick, the American ambassador.

THE Nichols medal of the American Chemical Society has been awarded to Thomas Midgley, Jr., head of the fuel section of the General Motors Research Corporation laboratories at Dayton, Ohio. The medal will be presented in New York on March 9 by Dr. C. A. Browne, chairman of the New York section. Professor Wilder D. Bancroft, of Cornell University, will deliver an address on Mr. Midgley and his work. Mr. Midgley will review the effects of anti-knock compounds on gaseous detonation.

AT the meeting of the Franklin Institute on February 21, Dr. Lee de Forest received the Elliott Cresson gold medal awarded to him for his invention of the three-electrode audion. In presenting Dr. de Forest for this award, his invention was characterized as one of the most important ever made in the field of the electrical transmission of intelligence and one which through its development has marked a profound revolution in the art of radio communication.

Two hundred members of the California section of the American Chemical Society attended dinner at the Hotel Bellevue, San Francisco, in honor of Professor E. C. Franklin, president of the American Chemical Society. Dr. Carl L. Alsberg, local chairman, presided, and Professor John M. Stillman was toastmaster. Addresses were made by Professors Leuschner and O'Neill, of the University of California; C. E. Grunsky, representing the Engineers' Society, and Dr. Alonzo E. Taylor and William L. Burdick, of Stanford University. Congratulatory letters and telegrams from many educators and chemists were read.

THE trustees of the University of Wyoming have passed the following resolution: "Be it resolved, that in recognition of the loyal and efficient service given to the University of Wyoming during the past five years by Dr. Aven Nelson, we, the members of the board of trus-

tees, express our appreciation of his untiring efforts toward the development and growth of this institution, and that we do hereby confer upon him the title of president emeritus of the University of Wyoming." Dr. Nelson is resuming his work as professor of botany.

As a result of the International Engineering Congress in Rio de Janeiro, the following North American engineers were elected to membership in the Club de Engenharia; *Corresponding members:* Josiah E. Spurr, editor of the *Engineering and Mining Journal-Press;* Louis J. Hirt, engineer, Pearson Engineering Corporation. *Honorary members:* Verne L. Havens, director *Ingeniería Internacional;* A. W. K. Billings, Canadian and General Finance Committee, Ltd.; Edward Wegmann, consulting engineer; Samuel M. Vauclain, president Baldwin Locomotive Works; Calvin W. Rice, secretary of the American Society of Mechanical Engineers.

Dr. Rudolph Matas, of New Orleans, professor of surgery in Tulane University of Louisiana, has been notified of his election to membership in the Royal Academy of Medicine, Barcelona, Spain. He has also recently been elected a corresponding member of the Société Nationale de Chirurgie de Paris.

The *Journal* of the American Medical Association reports that Dr. Juan Guiteras is living a quiet, rural life since his retirement from the charge of the national public health service in Cuba. On the occasion of the recent Sixth Latin-American Medical Congress at Havana, the Sociedad de Estudios Clinicos presented Dr. Guiteras with a gold medal as a tribute to his distinguished services to science. The address on the presentation of the medal was by Dr. Vicente Pardo Castello, followed by one of the delegates from Brazil to the congress. They spoke of Dr. Guiteras's work on yellow fever and his trip to Africa in 1916 to study epidemics under the auspices of the Rockefeller Foundation, and especially of his share in reducing the death rate from tropical diseases, not only in Cuba but also throughout the world.

The Italian Association of General and Applied Chemistry has appointed a committee under the presidency of Senator P. Ginori Conti, to arrange a celebration in honor of the seventy-fifth anniversary of Professor and Senator E. Paternó. As a permanent memorial of the occasion, the committee has decided to establish a memorial fund for the "Paternó medal" which will be conferred annually upon the author of the most important contribution to chemistry, regardless of his nationality. In order that the endowment of this medal may be given an international character, the committee welcomes contributions from the chemists of the world in support of the medal. Subscriptions from American chemists may be forwarded to the Division of Chemistry and Chemical Technology of the National Research Council, Washington, D. C., for transmission to the Italian committee.

A Spanish journal gives a list of more than one hundred towns in Spain which have renamed a street in honor of Professor Ramón y Cajal. Some have installed large artistic tablets to designate the street.

Professor Heubner, the senior German pediatrician, who for many years was head professor of pediatrics on the University of Berlin Medical Faculty, and who, since his retirement, has been living in a suburb of Dresden, celebrated on January 21 his eightieth birthday.

Owing to a severe illness arising from a wound received during the war in France, Sir T. W. Edgeworth David has resigned his position as president of the Australian National Research Council. His place has been filled by the election of Dr. Orme Masson, professor of chemistry in the University of Melbourne. Professor David continues to serve the council as vice-president.

Dr. Hubert Work has been transferred from the office of postmaster general to become secretary of the interior, succeeding Albert B. Fall, whose resignation became effective on March 4.

A joint resolution has been passed by the Senate and the House of Representatives reappointing Mr. Henry White as a regent of the Smithsonian Institution, appointing Mr. Frederic A. Delano to succeed the late John B. Henderson, and Mr. Irwin B. Laughlin to succeed the late Alexander Graham Bell.

The Rockefeller Foundation announces that it has elected to its board of trustees Dr. Ray

Lyman Wilbur, president of Stanford University, California, and William Allen White, writer and newspaper editor, of Emporia, Kansas; and to membership on its International Health Board, Dr. David L. Edsall, dean of the Harvard Medical School.

MR. GEORGE W. HOOVER, chief of the Chicago station of the Bureau of Chemistry, has been appointed chief of the newly created Drug Control Laboratory. Mr. M. W. Glover, who has been in charge of the office of drug administration, has been recalled to the U. S. Public Health Service, and Dr. L. F. Kebler, chemist in charge of the Division of Drugs, has been promoted to the position of chemist in charge of special collaborating investigations and will direct the work involved in the enforcement of the Postal Fraud Law.

O. D. STREET, general manager of distribution of the Western Electric Company, has been elected a vice-president of the McGraw-Hill Company. He will have executive charge of *Electrical World, Electrical Merchandising, Journal of Electricity and Western Industry, Industrial Engineer, Electric Railway Journal* and *Bus Transportation*.

DR. H. C. COOPER, formerly in charge of physical chemistry and electrochemistry at the College of the City of New York, has removed to Chicago to become director of the scientific department of Bauer and Black, manufacturers of surgical dressings, etc.

W. F. LANTZ has been appointed chief chemist at the plant of the Bethlehem Steel Company, Bethlehem, Pennsylvania, to succeed A. D. Shankland.

W. D. RICHARDSON, chief chemist, Swift and Company; Paul Budnick, chief chemist, Armour and Company, and L. M. Tolman, chemical director, Wilson and Company, constitute a committee of the Institute of American Meat Packers to find a chemist qualified for the directorship of scientific research of the institute.

THE division of chemistry and chemical technology of the National Research Council, Washington, D. C., has named Dr. James R. Withrow, professor of industrial chemistry at the Ohio State University, chairman of the National Research Council committee on the "Use of sodium compounds as a substitute for potassium compounds, both in scientific and industrial work." Professor Withrow declined the position as chairman of this committee four years ago because of the pressure of university work.

DR. MAXIMILIAN TOCH has been invited to deliver a course of lectures as honorary professor of industrial chemistry at the University of Peking and has tentatively accepted for the latter part of 1923 or the beginning of 1924.

P. VAN DE KAMP, chief assistant at the Observatory at Groningen, Holland, has arrived at the University of Virginia, where he will start a year's research at the Leander McCormick Observatory.

DR. MICHAEL SIEDLECKI, formerly the rector of Vilon University and now a professor of zoology at Cracow, has been invited to lecture at Vassar College during the absence of Professor Cora Beckwith, who has been granted a year's leave of absence.

ST. LOUIS UNIVERSITY announces a series of lectures on "The physiology of the lymphatics" to be given by Dr. Leon Asher, professor of physiology at the University of Berne. These lectures, five in number, will be delivered from March 5 to 9.

DR. E. V. COWDRY, of the Rockefeller Institute for Medical Research, will deliver the eighth Harvey Society lecture at the New York Academy of Medicine, on Saturday evening, March 10, 1923. His subject will be "The significance of the internal reticular apparatus of Golgi in cellular physiology."

PROFESSOR DANIEL STARCH, of Harvard University, gave on January 20 an address at the United States Naval Academy at Annapolis on "The use and limitations of psychological tests."

DR. COLIN G. FINK, head of the division of electrochemistry of Columbia University, gave an address on the "Corrosion of the nonferrous alloys," at the meeting of the New Jersey Chemical Society on the evening of February 12.

DR. PAUL D. MERICA, director of research of the International Nickel Company, addressed a joint meeting of the Minnesota Sec-

tion, American Chemical Society and the Northwest Chapter of the American Society for Steel Treating on February 19 at the Manufacturers' Club of Minneapolis. His subject was "Nickel, its metallurgical and industrial applications."

ON February 16 and 17, Dr. C. E. Seashore, of the University of Iowa and the National Research Council, conferred with the faculty of the University of Kentucky on the problems connected with the encouragement of scholarship among college students. Dr. Seashore also lectured at an open session of the Kentucky Chapter of Sigma Xi on his work in the study of musical talent.

DR. E. D. BALL, director of scientific work of the United States Department of Agriculture, gave a lecture on "The insect transmission of plant diseases" before the Journal Club of the department of medical zoology of the School of Hygiene and Public Health, Johns Hopkins University, on February 23.

DR. GEORGE T. MOORE, director of the Missouri Botanical Garden, delivered the following addresses at Cincinnati on February 12 and 13: Before the Blue Hydra Society of the University of Cincinnati, "Botany and business;" before the Cincinnati Chamber of Commerce, "The relation of the botanical garden to business and industrial life," and before the Cincinnati Chapter of the Wild Flower Preservation Society, "The relation of a botanical garden to civic life, as illustrated by the Missouri Botanical Garden."

PROFESSOR ALBERT STOWELL FLINT, astronomer emeritus of the Washburn Observatory, University of Wisconsin, died on February 22, aged sixty-nine years.

DR. WALTER S. HAINES, for forty-six years professor of chemistry in Rush Medical College, died on January 27, aged seventy-two years.

DR. JAMES RITCHIE, Irvine professor of bacteriology at the University of Edinburgh, has died at Edinburgh, aged fifty-eight years.

Nature reports that "the centenary of the death of Edward Jenner on January 26, 1823, was celebrated by the Academy of Medicine in Paris on January 23. A large meeting was held at the academy in the Rue Bonaparte, when the president, M. Chauffard, gave a short address, followed by a speech by M. Lucien Camus, and by communications on the subject of vaccination from MM. Pierre Teissier, Jeanselme, d'Espine and Sir St. Clair Thomson. The busts of Jenner and Pasteur were placed on the right and the left of the platform. After the ceremony a number of mementoes of Jenner in the form of letters by him, and of old cartoons commemorating or deriding vaccination, were shown in one of the halls of the academy. Sir Ronald Ross, a foreign associate of the academy, who represented the British Ministry of Health, handed in also a letter from the president of the Royal Society. Sir Almroth Wright, another foreign associate of the academy, was also present. After the ceremony the president and council of the academy, in honor of the commemoration, gave a dinner."

THE herbarium and library of the late Professor Elias J. Durand, who at the time of his death was chairman of the botany department of the University of Minnesota, have recently been purchased by Cornell University to be installed in the department of plant pathology of the New York State College of Agriculture. The herbarium, which contains over 12,000 specimens, consists almost exclusively of discomycetes, the group on which Dr. Durand was a recognized authority.

THROUGH the generosity of the Botanical Society of Pennsylvania purchases for the botanical department of the University of Pennsylvania have been made from Europe of valuable collections of Cryptogams, especially Bryophytes. These increase manyfold the usefulness of this part of the herbarium, previously insignificant. Additions of this nature approximate 4,000 specimens, with more to come. Further collections include donations from the universities of Copenhagen and Upsala; flowering plants from Scandinavia by Dr. J. W. Harshberger, from Mt. Rainier and the Selkirk Mountains by Dr. R. E. Cleland, from the Northwest Territories by Mr. R. Holroyd, and Algæ and Bryophytes from the Selkirk Mountains and the Bay of Fundy region by Dr. W. R. Taylor.

AN International Geographical and Ethnological Congress will be held in Cairo, Egypt, in 1925.

AN International Air Congress will be held

in London from June 25 to 30 at the invitation of the British government under the presidency of the Duke of York. The Air Ministry has arranged a pageant on June 30.

UNIVERSITY AND EDUCATIONAL NOTES

ACCORDING to a new agreement between the board of permanent officers of the Yale School of Medicine and the corresponding boards of Yale College and the Sheffield Scientific School, admission to the combined course in either undergraduate school will be on a competitive instead of an automatic basis as heretofore. Those seniors will be eligible as candidates for the course whose scholarship standing during the junior year has been seventy-five or above, and who have satisfied the science and language requirements of the medical school. Their applications, which are due before July 1, following the completion of junior year, will, however, be considered in conjunction with all other applications, and the medical class chosen from the entire number of candidates. The Yale undergraduates who are admitted will take the entire first-year medical work and receive credit for the B.A. or B.S. degree. The degree of M.D. is awarded after three years' additional study. As only sixty students are admitted each year from several hundred applicants, the decision to select all members of the first year class in the Yale School of Medicine on the same basis will interest premedical students throughout the country.

DR. W. A. WHITE, superintendent of the Government Hospital for Insane at Washington, D. C., announced the establishment of a school at St. Elizabeth's for the instruction of physicians in the treatment of mental and nervous diseases.

PROFESSOR D. H. DAVIS, of the department of geology of the University of Michigan, has accepted a professorship in the University of Minnesota.

DR. EDWARD A. DOISY has been appointed professor of biochemistry at St. Louis University, the appointment to take effect on August 1, 1923. Dr. Doisy is at present associate professor of biochemistry at the Washington University School of Medicine.

DR. J. C. M. BRENTANO has been appointed lecturer in physics at Manchester University.

DISCUSSION AND CORRESPONDENCE

THE FEEDING POWER OF PLANTS

IN a recent paper in SCIENCE[1] under the caption quoted above, Mr. Truog elaborates and supplements the discussion of his theory on the "feeding power of plants" as detailed in earlier publications.[2] His most recent discussion, as well as his earlier ones, are open to such serious question in many important respects that we deem it essential to comment on the principal objections to his theory and his assumptions. Several minor points in the paper first cited above which are open to serious question are not discussed in this review because of the limited space available (in this paper) and because the importance of the points which we do discuss should not in any wise be dimmed by matters of lesser magnitude.

Mr. Truog makes the fundamental assumption that each and every plant species, or group of plants, is characterized by what we may term a specific avidity for the ions in solution in the root medium, and that such specific avidity is contingent upon the reaction of the cell sap and the behest of the law of mass action. This assumption is, in turn, based on the alleged or actual percentages of the various ions found in different kinds of plants. This implies, of course, the fundamental power of the individual root cell to absorb ions in a characteristic manner. In all this discussion, Mr. Truog evidently underestimates the fact that the differences between plants which are in question may be caused, not by the difference in the specific absorbing powers of individual cells, say of legumes and grasses for example, nor yet by the specific reaction of the cell sap, but by the difference in extent of root system, and the difference in the amount and intensity of CO_2 production by roots. The enormous differences which obtain among different kinds of plants as regards the extent of

[1] SCIENCE, N. S., 56, 1922, pp. 294-298.

[2] SCIENCE, N. S., 41, 1915, pp. 616-618. *Res. Bull.* 41, 1918, Wis. Agr. Exp. Sta. *Soil Science*, 5, 1918, pp. 169-195, and others there cited.

root are too well known to the student of plants to need discussion, and they can scarcely be considered as of anything but profound importance in the mechanisms of absorption of ions by plants. But, dependent in part upon the extent of the root system, is the degree of CO_2 production, and the latter must play a dominant rôle in making available to the root cells the ions contained in the undissolved compounds of the soil particles. That this is not mere speculation is evident from the striking data adduced by Turpin[3] on CO_2 production by the roots of different plants and the importance thereof in the total amount of CO_2 produced in soils. It is even more strikingly supported by recent investigations of J. D. Newton in this laboratory, which have not yet been published, but which show a marked difference between peas and barley as regards the intensity of CO_2 production per unit weight of roots or tops under controlled conditions in sand cultures. Newton's results indicate further that legumes, as such, are not characterized by a greater Ca content than non-legumes, as such. For example, he found no significant difference between the Ca content of barley, peas and vetch, in solution cultures in which the plants were grown separately or together in the same solution. This was true in solutions of low and of high Ca content. Between beans and barley, the difference appears to have been greater in favor of the beans. On the other hand, in soil cultures with barley and peas grown together, the peas take up much more Ca than the barley. This supports the contention which we have made above that the intensity of CO_2 production, as well as the extent of the root system, may determine the amounts of ions absorbed independently of the specific power of plant species, genera or families in that regard. In this connection, it must be further emphasized that the nature of the environment of the plant, both as regards root and top, may play as important a rôle as any factor in inducing a certain rate and degree of absorption of ions. For example, Waynick[4] showed in experiments in this laboratory that the wheat plant varied *enormously* in its power of absorbing the common ions of culture solutions, depending on the constitution of the solution and the presence or absence of effective quantities of unessential ions. Our studies and observations on individual variabilities in plants in regard to their chemical composition, as in other respects, constrain us, moreover, to say that no justifiable conclusions on specific absorbing powers of different kinds of plants may be drawn on the data available to date. This observation is particularly cogent as regards the attempt to classify the plants supposedly requiring much calcium in accordance with their power to "feed" on calcium and phosphorus. The data employed for this purpose were derived chiefly from Wolff's "Aschenanalysen" and those represent data gathered from many men in many places. Since variability is so marked in plants of a pure strain grown under carefully controlled conditions, what can we expect in the case of Wolff's data but a degree of variability which invalidates the drawing of fundamental conclusions from them? The importance of the variability of the soil, as well as of the plant, as investigations of this laboratory have amply demonstrated, can not be overemphasized in connection with such conclusions as Mr. Truog attempts to draw from ash analyses and similar data.

Attributing to an essential element one or more kinds of functions in living cells is a procedure followed without more than the most slender evidence in its support. Highly speculative as this procedure may be, however, it becomes particularly questionable when the functions of elements like Ca and K are supposed to rest upon diametrically opposed bases.

There are other points in the paper under review which require even more critical examination than the one in the foregoing paragraph. We can not conceive of the application of the law of mass action to anything but definite chemical reactions between definite reacting substances and hence there is little likelihood of gleaning anything from an application of the law such as Mr. Truog makes. The reactions in question do not lend themselves to such simple treatment. We deal there unquestionably with complicated dynamic phenomena, which, in turn, may involve certain mass ac-

[3] *Research Memoirs Cornell Agr. Expt. Sta.*, Mem. 32, April, 1920.

[4] *Univ. Calif. Publ. Agr. Sciences*, Vol. 3, No. 9, p. 243, July, 1918.

tion phenomena, but clearly they are not such as to permit of the simple interpretation given, as even casual consideration of their nature will reveal. It is assumed, for example, that K is precipitated in the living cells through its interactions with the chemical constituents in those cells so as to permit the constant flow of K in solution into the cell from the medium. But any one who has studied the composition of cell sap from a variety of plants can not help but be impressed by the enormous quantities of soluble (not precipitated) K salts which the sap contains and, in all likelihood, in the form of *simple dissociated chemical compounds*. The data obtained on kelp and on other algæ, and those obtained by Kostychew and Eliasberg[5] on other plants, as well as unpublished data recently obtained by Hoagland on cereals, and by Hoagland and Davis on Nitella (a plant used by Osterhout in permeability studies), support the significance and cogency of this statement. Since space forbids our considering several examples, let us study the results on Nitella. This simple alga absorbs K readily from exceedingly dilute solutions thereof and from solutions having a high p_H value to a region within the cell of lower p_H value. This, in itself, proves Mr. Truog's assumption to be fallacious. Hoagland and Davis have found further, in agreement with Osterhout and Crozier that that alga lives in a medium whose p_H varies from 7.2 at night to 9.4 during the day, yet the hydrogen ion concentration of the vacuolar sap of that plant is represented by an approximately constant value of p_H 5.2. Further, Nitella lives in a solution whose Cl content varies from 20 to 30 p.p.m., yet the vacuolar sap of the alga has a Cl concentration of approximately 3,500 p.p.m. Moreover, in suitable media, all the Cl may be taken up by the alga and yet none will pass out of the cell unless the latter is injured, or the normal permeability is changed. Does this in any way support the attempt to apply the law of mass action to absorption of ions by plants after Truog's concept? The general principles gleaned from the work with Nitella should be applicable to plants generally.

[5] Hoppe-Seyler's Ztschr. physiol. chem., Vol. III, pp. 228-235 (1920).

It is, of course, regrettable that this review must be curtailed and that, therefore, a fuller discussion can not be accorded the points which are considered above, and some discussion to important ones which have not been mentioned. Enough has been said, however, to indicate the insecurity of Mr. Truog's theory and of the assumptions upon which it is based. The indications from recent research are that the reaction of plant sap in nearly all plants that have been investigated varies only in a very narrow range (Haas' work on natural indicators being exceptional), and that the conclusion that the ability of plants to absorb K from their media depends on the reaction of the cell sap and that sap of a high p_H permits plants to absorb K more efficiently from dilute solutions thereof than that of a low p_H are probably erroneous.

We may mention, further, in passing, that experimental evidence has been adduced in this laboratory to show that plants do not require Ca in the form of carbonates or bicarbonates since they can grow in solutions so acid that such compounds can not exist to the extent considered necessary.

Finally, we may say in closing that the term "feeding power of plants" is not only insecure, because of its vague support in fact, as partly indicated above, but also because absorption of ions is in no correct sense a feeding by plants and the sooner we give up referring to the mineral elements in soils as plant foods and of speaking of culture solutions as nutrient solutions, the better it will augur for the attainment of that clarity of expression, as well as thought, which are requisite to constant progress in plant physiology, as in other sciences.

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THE PERSONAL EQUATION OF THE ASTRONOMERS

THE data reported by Tucker in his communication on "Reaction time and fatigue"[1]

[1] Tucker: SCIENCE, LVII, No. 1468, February 16, 1923, pp. 204-5.

does, as he supposes, interest psychologists, but mainly because his results fall within the range of details which have been pretty thoroughly worked out by psychologists. Burrow's monograph[2] contains the best historical account of the investigations up to 1909, and very little has been done on the problem since the publication of my own articles in 1910.[3]

Although the causal factors have not yet been precisely determined, certain points are clearly settled. First, the "personal equation" is not a matter of reaction-time at all, but a matter of the synchronization of reactions and anticipated stimuli. Simple reactions to visual stimuli are seldom shorter than 100σ (0.1 sec.), whereas the errors in synchronization are usually much smaller and frequently negative. The errors fall within the limits of the imperceptible time interval for the conditions of observation. Second, the usual error, without practice, is negative, and practice tends to change it towards the positive. Third, the same phenomena appear under conditions in which no overt reaction is required, but in which, as in the astronomer's eye-and-ear method and Wundt's complication experiment, the subject is required merely to note the apparent position of a moving object with reference to a series of auditory stimulations. And we are now safe in saying that this is because the latter cases are of the same type as the former, that is, that a discriminative perception is really a reaction: and this in a literal, not a figurative sense.

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QUOTATIONS

COPERNICUS

THE whole world should observe along with the Poles the birthday of Copernicus, and should continue to celebrate the 19th of February in his memory so long as the earth swings in its orbit; for what this boy, born 450 years ago, and christened Nichola Koppernigk, son of a native of Cracow, conceived as the order of the universe is "the capital event of modern thought." By it mankind's outlook on the universe has been fundamentally changed. The young Koppernigk was a student in the University of Cracow the year in which Columbus discovered America, giving himself to mathematical science and painting. He afterward studied law and attended mathematical lectures in Bologna and still later studied music in Padua and took his degree in canon law in Ferrara. He then devoted his medical skill to the service of the poor, his economic knowledge to the reform of the currency in the Prussian provinces of Poland, and his astronomical genius to the development of a new cosmic theory which has come to bear his name.

It was while he was in the midst of such studies and ministries that the name "America" was first given by others to the fringe of this continent and graven on a map published at St. Dié, at the foot of the Vosges Mountains, in southeastern France. Our continent was thus christened under the Ptolemaic geocentric system. But our national life made its beginnings under the Copernican system and had from the first a "shuddering sense" of physical immensity. It is inconceivable that this new physical conception has not mightily affected man's social and religious conceptions, and especially those of Americans. With the enlargement of the universe under it, and the accompanying diminution of the relative size of the earth—made still smaller by man's improved means of communication—we no longer picture our planet as a flat area divided into exclusive, provincial or national strips spanned by a Ptolemaic sky. We find ourselves "in the same boat" on a sea of practically infinite space.

In observing the birthday of Copernicus the Polish astronomers have fitly gathered in their first congress and proudly remembered what their science has given to mankind; and the Polish people have with good reason held their celebrations all over Poland in honor of the son of the city of Thorn (now again in Polish territory) and the academic son of the

[2] Burrow: "The determination of the position of a momentary impression in the temporal course of a moving visual impression. *Psychol. Monographs*, XI, No. 4.

[3] Dunlap: "The complication experiment and related phenomena," *Psychol. Review*, XVII, pp. 157-191; "Reactions to rhythmic stimuli with attempts to synchronize," *Psychol. Review*, XVII, pp. 399-416.

University of Cracow (once more a Polish university). But it would be profitable for the whole world—scientists, statesmen, warriors, philosophers, teachers, pupils and the people in general—to pause and consider what was the real significance of the gift of Copernicus. The corollary of his theory is a world-wide solidarity of human interest. There is no escape from it. If an international holiday were to be added to the many holidays in the various calendars of the world, it should be one on which the birthday of Copernicus is solemnly observed—for he discovered the universe.—*New York Times.*

SCIENTIFIC BOOKS

The British Association for the Advancement of Science: a Retrospect, 1831-1921. By O. J. R. Howarth, O.B.E., M.A., Secretary. 14 x 22 cm., vii + 318 pages, numerous illustrations. Published by the Association, Burlington House, Piccadilly, London, W.1, 1922.

American men of science should find much to command their interest in the attractive volume here cited. The book, which is a very readable summary of the development and achievements of the British Association, is replete with information and suggestion for those who hold membership in our scientific societies and in the American Association for the Advancement of Science—especially for those whose hope for our future rests partly in efforts toward the strengthening of American scientific organization.

In these days when the human value of historical appreciation is so strongly and so rightly emphasized on every hand, when the development of the sciences seems almost on the point of becoming subject matter for a new branch of historical science, it is particularly fortunate that Mr. Howarth has been able to present us with this account, which is in some respects a résumé of the history of British science since 1831. The association was founded in that year. The war that finally ended with Waterloo had left the British people with new insight into possibilities for national improvement. Somewhat parallel to what is happening to-day, many minds became constructively critical and there was clamor for many kinds of reform. Among the reformers were leading men of science, who complained of the "decline of science in England" and advocated an association of scientific men as a means toward the attainment of the improvement that was sought.

The British Association was organized at York, on September 26, 1831. The Reverend William Vernon Harcourt, chairman of the organizing committee, proposed the foundation of an association "having for its objects, to give a stronger and more systematic direction to scientific inquiry, to obtain a greater degree of national attention to the objects of science, and a removal of those disadvantages which impede its progress, and to promote the intercourse of the cultivators of science with one another, and with foreign philosophers." So the association came into existence. It was largely modeled after the Deutscher Naturforscher Versammlung, which had held its first meeting at Leipzig in 1882. That organization is now the Gesellschaft Deutscher Naturforscher und Ärzte. We may note further that our American Association was principally modeled after the British, when the latter was seventeen years old.

In a chapter on "The Association and the Progress of Science" is, among others, an excellent account of the two famous controversies between science and religion through which the British Association passed, the first following the general appreciation of the meaning of Lyell's "Principles of Geology," the second following the appearance of Darwin's "Origin of Species." To American scientists these pages are now specially interesting, for history does seem to repeat itself in our midst.

Throughout its history the British Association has ever held strongly to its aim of bringing science to the non-scientific, to society at large, especially to the public of the cities in which the annual meetings are held. General addresses have frequently been given that "demonstrate to all men that science is thinking with and for them, about matters which must interest and most deeply concern them."

The association has met every year since its foundation, with the exception of the two years 1917 and 1918. It has met at thirty-four cities in the British Isles and has held meetings over-

seas on five occasions—in Canada three times and in Australia and South Africa each once. The York meeting for 1881 was a jubilee, the fiftieth anniversary of the original York meeting. An appendix to the volume presents dates and places of all the meetings, together with biographical notes on the presidents.

Besides holding the annual meetings, the British Association has been active in the initiation of many pieces of research and in the support, through grants, of very many more. A list of grants made in support of research, for the period from 1834 to 1921, is given in an appendix. The association has successfully called the attention of the government to various scientific needs from time to time. It has received some government help, but "to voluntary service in the interests of science the whole record of the British Association stands as one great memorial. Every word spoken at its meetings, every page of its annual reports, represent voluntary effort on the part of individual 'cultivators of science.'"

Burton E. Livingston

SAFRANIN AND METHYL GREEN

In a report last summer made by the Committee on the Standardization of Stains it was stated that good success had been obtained in securing satisfactory samples of almost all the important biological stains with the exception of safranin and methyl green. It is very encouraging now to be able to report satisfactory sources of both of these stains.

Safranin has proved a difficult stain to secure in exactly the right quality, because one is desired which will contrast with both gentian violet and with orange G when used in the Flemming triple stain. The products on the market two years ago when the first samples were collected were generally the textile safranins without much modification and proved to be of too blue a shade to contrast with gentian violet. A sample of Grübler's safranin was compared with them at that time which seemed to give good results, and one manufacturer agreed to duplicate this for the committee. This particular Grübler sample proved, however, to be a mixture of safranin and auramin; and when the American manufacturer made a similar mixture, the resulting shade was found by some of the investigators to be too yellow to contrast with orange G. In one of the laboratories where it was tested, however, it was found to be entirely satisfactory for the particular purpose for which it was used, and it is felt that this mixture may well have its value but should not be sold as a safranin. The matter was in this state when it was finally referred to the National Aniline Company. They have now prepared a safranin which is very pure and considerably more concentrated than any which has before been put on the market. It has been reported upon in the highest terms from every laboratory except the one where best results were obtained with the mixture of safranin and auramin. It seems to give satisfactory results in the Benda stain with light green, also in contrast with both orange G and gentian violet and appears to be identical with the best of the pre-war Grübler safranin O, except that it is more concentrated.

Methyl green is used quite largely in the Pappenheim stain together with pyronin; but it is also used by botanists in staining plant tissue and by zoologists as a chromatin stain, and for staining living protozoa. The first samples of this stain obtained in this country were apparently either methylene green or methyl green with violet impurities, probably methyl violet. Of the samples obtained in the original investigations of the committee none proved very satisfactory; but last summer a sample was obtained from the Providence Chemical Laboratories of Providence, R. I. which has proved as satisfactory as the samples of Grübler's methyl green with which it has been compared. More recently a series of three samples, each one purer than the preceding, have been sent to us from the National Aniline Company. The first sample proved unsatisfactory; the next one submitted proved of good quality, while the most recent sample submitted seems to be one of the purest samples obtainable. There is still some question, however, which of these methyl greens is actually the most satisfactory when judged from the standpoint of performance. Although one laboratory has reported in highest terms of the purest of these samples, two other laboratories found best results with one of the

less pure methyl greens. It is not impossible that some of the impurities present in Grübler's methyl green may have been of special value in staining. If this is the case, it is plainly the problem now for biologists in cooperation with the manufacturers to find out what these particular impurities were. Meanwhile it seems safe to recommend any one to buy a methyl green from either of these two companies, in as much as their methyl greens are evidently as good as any obtainable at the present time.

The findings obtained in this investigation have been very interesting in showing the amount of variation in different lots of Grübler stains. Before obtaining the satisfactory American sample of safranin O, mentioned above, data were secured concerning the properties of two different Grübler samples. One proved to be actually safranin O, but with considerable inert material (dextrin). The other proved, as stated above, to be a mixture of safranin and auramin, and contained fully 90 per cent. dextrin. Although considerable variation has been found in the samples now on the market in this country, no two samples of safranin have been found differing from each other to such an extent as these two samples, one imported before the war, the other during the war.

Attention is called to this fact to show that standardization can not be obtained by going back to imported stains. There is, moreover, very good reason to believe that the imported stains now on the market are still different. Investigation of this point is in progress at present. Meanwhile we have learned of one well-known German laboratory which has just ordered stains from America, with the comment that the European products are now very unsatisfactory.

H. J. CONN
Chairman, Commission on the Standardization of Biological Stains
GENEVA, N. Y.,
FEBRUARY 20, 1923

SPECIAL ARTICLES

FURFURAL AS A BIOLOGICAL REAGENT

IN spite of its wide range of solvent power, preserving qualities and rapid penetration of tissues, furfural has remained until recently a chemical curiosity, its properties and even its appearance practically unknown even to many who have frequently employed it. Investigations concerning its uses as a biological reagent have been wholly confined to its physiological effects. This is perhaps due to the fact that until recently it has not been manufactured in commercial quantities, being little used in the industries. Now, however, the "Miner Laboratories" of Chicago have developed a process of manufacturing furfural from oat hulls which are obtained in large quantities as a by-product in several industries. At the same time several commercial uses have been developed in the manufacture of resins, varnishes, rubber, dyes, perfumes, anesthetics, antiseptics and germicides. Thus larger demand and increased efficiency of manufacturing process have made possible the reduction in price to a point where it bids fair to become the cheapest aldehydic body commercially available.

Furfural is an aldehyde of deep amber color and a pungent odor, having the formula C_4H_3OCHO, which, in general, resembles the reactions of benzaldehyde on the one hand and formaldehyde on the other. It differs from either of them in being a liquid of very high boiling point and wide range of solvent power. As a biological reagent its chief interest lies in the fact that its properties closely resemble those of formaldehyde, suggesting its use as a preservative, and secondly its wide range of solvent power including most of the coal tar dyes, hæmatoxylin, alcohol, xylol, toluol, benzole, balsam and parlodion, suggesting its possibilities as a vehicle for stains as well as a general reagent in micro-technique; and thirdly, its reaction with acids forming resins, suggesting its use as an injection fluid.

In connection with current laboratory procedure we have had occasion to test out furfural in these three fields and the results are herewith reported.

The qualities of furfural as a preservative were investigated for a period of four months on several animals used in the laboratory for dissection purposes, such as cats, frogs, fish, mussels and some plant forms. Although H. McQuigan ('22) has shown the germicidal and toxic properties of furfural to be less than that of formaldehyde it has nevertheless been adequate to preserve these specimens in good

condition. Two strengths of solution were used, ninety-five per cent., or commercial furfural, and a seven and one half per cent. aqueous solution, both of which have proved successful. Furfural is soluble in water only to the extent of eight per cent. at room temperature. We therefore used a solution slightly below the point of saturation. This strength of solution seems to be preferable as it does not discolor one's hands or the tissues to so great an extent and the odor is not as objectionable as with the commercial furfural. Such stock solutions for preserving purposes are most conveniently prepared by adding five pounds of furfural to a carboy of water and decanting off the saturated solution as it is needed, refilling with water until the furfural has been used up.

In each case the body cavity was opened so that the viscera were well exposed to the solution. After one to three months the specimens examined showed organs in a good state of preservation and did not show as great shrinkage or hardening as in those preserved in formaldehyde. In the case of frogs and mussels, the viscera were preserved in nearly as plastic a state as when living.

In general it has a tendency to discolor the tissues a yellowish brown, and the musculature of those specimens into which the ninety-five per cent. solution had been injected turned a deep brown color. This effect which would render furfural objectionable as a preservative for museum specimens is not very objectionable for dissection purposes. The mould Penicilium which here grows so profusely on formaldehyde solutions did not develop at all on the seven and one half per cent. furfural, although the solutions were repeatedly inoculated. Tested on pieces of muscle it also showed more rapid penetrating qualities than in the former. Although it has a somewhat pungent odor, its low vapor tension renders it less offensive than formaldehyde and in the seven and one half per cent. solutions the odor is scarcely noticeable. The irritating effect on the nose and eyes, so objectionable in the case of formaldehyde solutions, is here eliminated entirely. The same may be said with regard to its effect on the skin, where, although it does cause a yellowish discoloration it does not have the tendency to harden and shrivel it. The discoloration can readily be removed by washing the hands in warm water to which has been added a little ammonia.

If an acid such as hydrochloric or sulphuric is added to furfural it forms a resin. The rapidity with which the reaction takes place depends to some extent upon the amount of acid used. One cubic centimeter of HCl to one thousand cubic centimeters of commercial furfural completed the reaction in about thirty-six hours. The resultant product was a semi-solid spongelike, jet-black resin. This reaction was made use of for injection purposes and rendered the arteries and veins very conspicuous. Cats were used for this test, which were injected through the femoral artery with a ninety-five per cent. solution of furfural to which had been added, just previous to injection, one cubic centimeter of HCl per one thousand cubic centimeters of the former. Such an injection fluid is much more conveniently prepared and injected than the usual injection mass and is also cheaper.

In the field of micro-technique furfural promises to be a very useful reagent because of its wide range of solubility. Its penetrating qualities, together with the fact that it dissolves the majority of the coal tar dyes, makes it possible to use it as a vehicle for these stains. Secondly, it is miscible with alcohol in all proportions and can be used as a substitute for the higher percents in the dehydration process. Thirdly, it is soluble in all the aromatic hydrocarbons such as benzol, toluol and xylol. The same is true with regard to the oils such as bergamot, cedar and clove, and lastly even gum damar or Canada balsam. Thus it shows a wider range of adaptability to the different formulas than the alcohols, with a corresponding shortening and simplifying of several of the processes. It is the most rapid solvent of Celloidin or Parlodin that I know of and makes possible the elimination of the slower and more expensive ether alcohol method. At present tests are in progress in this laboratory on these properties of furfural with a view to determining its adaptability to this phase of the work.

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